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Prof. Abner Shimony
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Dear Professor Shimony:

Thank you very much for your letter of Aug. 10/90. We (Prof. Redhead, Dr. Butterfield and myself) are glad that you are in complete agreement with us on the issues raised in our July 26/90 reply to your referee report. We are also happy to hear that you believe you would accept a revised version of our paper in accordance with the changes we proposed. We shall send this revision to Prof. van der Merwe, and also directly to you, in a few weeks time; and then we can take things from there.

Regarding the "essence of the GHZ theorem" that you refer to in your second paragraph: We agree that, "in essence", the GHZ theorem does not *require* an uncountable set of angles (indeed, showing that this is so was the initial motivation behind our paper). But one must distinguish two questions:

(A) Do GHZ (Mason Symposium), GHZ/Shimony (i.e. your recent joint paper), and Greenberger (i.e. his lecture notes), *in fact* claim to prove the constancy of result functions A, B, etc. for uncountably many settings?; and

(B) Do they *need* to invoke a proof with uncountably many settings?

Although our paper (and Mermin's, which relies on ours in this respect) establishes that the answer to (B) is 'No'; we argued in our July 26th response to you (cf. section III.(7)) that the answer to (A) is 'Yes'. It is not totally clear from what you say whether you would agree with us about (A). But what *does* seem clear is that your main concern is with question (B), rather than (A). We agree with this emphasis, and so shall not overplay the significance of question (A) at any stage in our revised manuscript.

With regard to your two suggestions in your paragraph 3, we find that our inclinations are rather different from yours regarding what should constitute the essence of our revised manuscript. Let us first comment on your suggestions, and then we shall point out why these fail to capture all that we achieved in our paper.

Concerning your suggestion (1): We are not aware of any specifically measure-theoretic questions that arise in our proof which are "interesting", other than the point we have already made about the invalidity of the proof when uncountably many settings are invoked. But paragraph 3, sentence 4 of your letter makes it sound as though there are others worth highlighting. Suggestions for other possible conceptual errors that could arise are welcome!

As for the undoubtedly interesting question of the physical plausibility of our assumptions (cf. your suggestion (2)), we agree that our treatment in section **2** of our paper was somewhat incomplete in the following sense. Our treatment does, we believe, go beyond previous discussions in the literature on this point - particularly in the way we justify our choice of spacetime regions in figure 1 so that our proof's assumptions are made as plausible as possible. (Pg. 9, paragraph 2 and note 8 point out the main differences between our approach and the ones adopted by Bell and Shimony *et al* in Dialectica.) But there remains the question of whether there are *other* ways of formulating our assumptions and/or choosing spacetime regions which are just as plausible; indeed, possibly *more* plausible (on some other measure of "plausibility"!). Butterfield's contribution to the Notre Dame volume *Philosophic Consequences of Quantum Theory* has already explored this question further, and so we felt it best to refer to that (cf. note 8 of our paper), rather than repeat the results of his exploration and risk eclipsing the new, more technical, results at the heart of our paper.

What results are these? We believe the essence of our paper lies in the following: (i) Generalizing the GHZ proof to a stochastic hidden-variables framework (cf. section **2**); (ii) Showing that, given the strict correlations and locality assumptions we invoke, this stochastic framework collapses, "almost everywhere", to a deterministic one where the various probabilities must be causally independent of apparatus hidden-variables (cf. Lemma 2); (iii) Providing a *finite* proof of the GHZ theorem, i.e. which manifestly only uses countably many settings - in fact just 5 sets (cf. eqns. (3.7a)-(3.7e)) (Mermin subsequently showed that this number reduces to 4 in the 3-particle case); (iv) Articulating a measure-theoretic flaw which arises in the proof if uncountably many settings are invoked (cf. section **4**); (v) Showing how the assumptions made in our proof are subtantially weaker than those required by the most general version of Bell's proof (cf. section **6**); and (vi) Showing that, even if the requirement of *strict* correlations is relaxed, a version of our theorem goes through and leads to an experimentally testable inequality (cf. section **7**).

The issues you raise in your suggestions (1) and (2), viz. the possibility of other interesting measure-theoretic errors and the physical plausibility of our assumptions, are in a sense peripheral to the above results. For these issues have to do with the foundations (both mathematical and physical, respectively) of our proofs, and not with the results established by the proofs themselves. For this reason, we must disagree with you in holding that suggestions (1) and (2) capture the "essential content" of our paper. Of course, this is not to say that the foundations of our proofs are not important; after all, a proof is no more interesting than the assumptions it rests upon! But we believe that what we've already said in section **2** of our paper, particularly about the plausibility of our assumptions, *suffices* to motivate our assumptions on the basis of 'locality' - although the question remains open as to whether this is the *best* motivation one can provide, as we've said above.

Finally, it is worth noting that our result (vi) has in effect been independently discovered by GHZ/Shimony in their joint paper. I (Clifton) checked and found that the inequality in eqn. (29) of that paper turns out to be the inequality we would have been led to by our reasoning had we been working with the 3-particle state discovered by Mermin, rather than the original 4-particle 'GHZ state'. You will notice, however, that our reasoning is rather different from yours in getting to an inequality.

There are two differences. First, both ourselves and GHZ/Shimony drop the EPR *argument* (as opposed to the EPR *program* - your distinction at the bottom of pg. 18), but we seem to drop it for different reasons. In our case, the EPR argument (more precisely, our measure-theoretic analogue of it - cf. Lemma 2) no longer goes through because we no longer invoke *strict* correlations. But in your case, it is not clear whether you have abandoned the strict correlations. For you say (pg. 18, paragraph 2) that "...premisses (ii), (iii), and (iv) without (i) do not suffice to make testable predictions" which suggests that you believe nothing experimentally interesting arises if strict correlations (your "premiss (i)") is dropped. If so, our derivation provides a counterexample to that belief. Furthermore, you motivate the derivation of your inequality by noting the "curiosity of the logical situation" (viz. that strict correlations initiate the EPR argument, and also suffice to prove the contradiction), and then arguing that this creates a difficulty from the experimental point of view. But we don't see the difficulty. Since premisses (i)-(iv) are inconsistent, it seems perfectly coherent to experimentally test premiss (i) (the strict correlations), and thereby falsify the conjunction of premisses (ii)-(iv). The only thing blocking this strategy is the experimental difficulty of realizing strict correlations, which is what motivated our own consideration of the "nonstrict correlations" case.

The other difference between our approaches is that the EPR program, which your 3-particle inequality aims to falsify, assumes deterministic "response" (or, as you say, "result") functions with no causal dependence on apparatus hidden-variables. In contrast, our argument

does not assume determinism, nor does it assume causal independence from apparatus hidden-variables. So it shows that experimental violation of our 4-particle inequality can falsify a weaker EPR program than the one you postulate. And we believe it really is weaker, despite the fact of Stapp and Fine having general proofs of the equivalence of stochastic and deterministicmodels (cited by GHZ/Shimony on pgs. 24 - 25). In brief our reason is this. Recall:- Fine's reduction of a factorable stochastic model - which gives probabilities for results A, B, etc. conditional on λ and quantities a, b, etc. (i.e. pr(A/λ,a), pr(A,B/λ,a,b), etc.) - to a deterministic model goes by defining the big joint p(A,A',...,B,B',.../a,a',...,b,b',...) in terms of the factorable stochastic model, as:

$$\Sigma_\lambda \, pr(A/\lambda,a)pr(A'/\lambda,a')\ldots pr(B/\lambda,b)pr(B'/\lambda,b')\ldots pr(\lambda)$$

He then easily shows in the usual way that this big joint is equivalent to a deterministicmodel: each λ is a sequence of results <A,A',...,B,B',...>, etc. We now say: Given the big joint, the deterministic model is uniquely fixed. But given the stochastic model, <u>many</u> big joints exist whose corresponding deterministic model delivers the same statistics as the stochastic model. (Fine's choice has the particular property that at each λ, any values A, B of any 2 quantities a, b are stochastically independent.) So we say: One is at liberty to deny the joints' physical reality - at least until someone gives an account of physically real probability which singles out just 1 big joint. (Butterfield discusses this in more detail in a forthcoming paper in *The British Journal for the Philosophy of Science*.)

By the way, thank you very much for refereeing our paper so diligently and quickly: we really appreciate your time and comments. Also, I (Clifton) did discuss questions (A) and (B) above with Anton Zeilinger in Finland and he seemed to agree with us, although it was hard to tell because he appeared to be more interested by the experimental side of things than with conceptual details. However, I did succeed in getting him to change an assertion on one of his transparencies which claimed that one could prove that the GHZ result functions were constant "<u>for all</u> ϕ": so I suppose that's some consolation!

I see you're taking part in the "State-vector reduction" session at the PSA conference in October. I shall look forward to seeing you there, and possibly discussing the above issues further. In the meantime, we shall forward our revised manuscript to you and await further decisions.

Yours sincerely,

Robert K. Clifton
(Written by myself, after consultation with Prof. Redhead & Dr. Butterfield)

I've fixed this!

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